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ON ERRORS OF OBSERVATION.[1]

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Currents of thought often arise at different sources, and flow on for a long way before they mingle. This has been the case with the investigation of errors of observation in physics and in psychology. On the one hand methods for securing the nearest approximation to the true value from discordant observations have been studied by many of the most eminent mathematicians and physicists since the revival of learning. On the other hand the accuracy with which the external world is perceived has always been a central subject in psychology, and in the development of experimental psychology no portion has received more attention than the perception and comparison of differences in intensity. It has, however, to a considerable extent been overlooked that physics and psychology are concerned with the same phenomena. This is not surprising, as the points of view of the two sciences are different. Physics seeks to eliminate errors of observation; psychology seeks to study their nature. But the time has now come when each science should profit from the progress of the other. Physical science can better eliminate errors of observation by learning what is known of their cause and nature. Psychology will gain greatly in clearness and accuracy by using the methods of physics and mathematics.

The errors of observation with which physics and mathe-

[1] Read at the meeting of the American Psychological Association, Philadelphia, 1892.

matics have dealt are variable errors, such errors as would occur were each error composed of a very large number of comparatively small and independent errors, equally likely to be positive or negative. In this case the average of the observations is the most likely value, and its approximation to the true value is measured by the dispersion of the errors, and increases as the square root of the number of observations. In two important respects the mathematical theory needs to be supplemented by psychological experiment. In the first place, constant errors are entirely beyond the range of the method of least squares, and yet these are evidently more dangerous in physical observations than variable errors. Thus, for example, in the case of the personal equation of the astronomers, the variable error of an observer can be reduced to any desired extent by increasing the number of observations. But it was found on comparing the observations of different observers that they had constant errors far more serious than their variable errors. It was (and apparently is still) thought that the constant error of an observer becomes a variable error when the observations of several observers are combined. It is very unlikely that this is the case. The uniformity of the processes of perception and movement is greater than their variability. We may feel confident that the combined personal equations of all the astronomers would be subject to a constant error which cannot be eliminated by physical or mathematical science. But such constant errors depend on fixed psycho-physical conditions, and can be measured by the psychologist.

In the second place it may be urged that the theory of probability can only give a rough and ready account of the distribution even of variable errors. In measuring an inch an error of a mile will not occur, and a negative error of a mile is inconceivable. The probability assigned to such errors by theory is, indeed, extremely small, but the same probability is assigned to positive and negative errors, and they are not equally likely. It would seem that as a rule positive errors are more likely than negative errors. In measuring an actual inch, a positive error of two inches might occur, a negative error of the same size cannot occur. In ordinary errors of observation a corresponding preponderance of positive errors may be expected, and a correction for such excess must be empirically determined. The same holds for the averages which are so widely used in statistics. Thus, if the average weight of men be 150 pounds, men weighing 300 pounds occur, men weighing 0 do not occur. The average is not identical with the median, as required by the theory of probability. The assumption made by the mathematicians,

that an error is composed of a very large number of comparatively small and independent errors, cannot be admitted by the psychologist. If the fiction of indefinitely small errors be accepted at all, the elemental errors cannot be regarded as independent, but are interdependent and occur in groups. The distribution of errors will not follow simple and universal formulæ, but the greater our knowledge the more complicated will the formulæ become, and they will be as numerous as there are observers and observations. The deductions of Laplace and Gauss are of the greatest importance, but it should not be forgotten that the laws of nature cannot be invented, they must be discovered. It is within the province of psychology to supply physics with the formulæ it requires for eliminating errors of observation in special cases.

Turning now to what psychology can learn from physics, we find that the variable error of the method of average error and the probable error (or *h* as used in Germany) of the method of right and wrong cases are the error of observation of physical science. We may ask, why should there be an error of observation? Why should not the same stimulus be accompanied by the same sensation? The natural answer is that the conditions do not remain the same. In the first place the stimulus itself cannot be kept exactly constant. Lights are always variable, and sounds and touches cannot be exactly reproduced. Temperatures and smells are especially inconstant. Weights may remain nearly the same, but the manner of lifting them is always different. We have, therefore, a variable stimulus which in part accounts for the variation in sensation. In the second place the nervous mechanism is constantly changing. The sense organ is rhythmically exhausted and restored, and is subject to various irregular alterations. The nerves and paths of conduction in the brain would transmit more or less of the energy of the stimulus according to their ever changing condition. Lastly, the brain centres immediately concerned with perception alter greatly in metabolism. These latter changes are best known to us on the side of consciousness; there is a more or less regular rhythm in attention, and very numerous irregularities due to fatigue, interest, inhibition, etc. These sources of variation will sufficiently account for the fact that the same sensation does not recur. They are, indeed, so numerous and to a certain extent so independent, that they justify roughly the assumption of the mathematician, and the results of experiments show that the errors are in a general way distributed as required by the theory of probability.

In psycho-physical experiment two magnitudes are perceived and compared. The combined error of perception

would be larger than a single error of perception, being the square root of the sum of the squares of the separate errors, or nearly the error in a single case multiplied by the square root of two. We have further the errors of memory and comparison. The analysis of these factors at the present time would be very difficult, but I believe they would simply increase the variable error of observation, and introduce additional constant errors. This is not the view taken by Fechner, Müller, Wundt and others, to whom we chiefly owe the development of psycho-physical research and theory. They maintain that there is a threshold of difference, and when sensations differ by less than this amount there is no difference in consciousness, Fechner does not question the application of the probability integral to the comparison of magnitudes,[1] on the contrary it was he who first applied it to the method of right and wrong cases. He argues that a difference in the stimuli smaller than the threshold might be made apparent in consciousness by the error of observation, and would give the preponderance of right cases required by theory. But in about one-seventh of his trials he was doubtful as to which of the weights used by him was the heavier, and holds that in these cases the difference in the weights and the error of observation combined fell within the threshold, and that there was no difference in consciousness.

Prof. Fullerton and the writer[2] made experiments with lifted weights similar to Fechner's. In one series of 3000 experiments in which the probable error was much the same as Fechner's, the observers were doubtful 23% of the time, but on guessing which of the weights was the heavier they were right 62½% of the time. This is the percentage of right cases required by the theory of probability, on the supposition that the differences in consciousness follow Gauss' formula, and we may conclude that the difference in consciousness always exists and affects the course of mental life, even when it is so small that it cannot be detected.

Another case in which German psychologists have run counter to the theory of probability is in the assumption of a just noticeable difference. According to the theory of probability the apparent difference in sensation and the probability of correct judgment tend to increase continuously as the difference between the stimuli is made greater, but it is

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[1]As implied by Peirce and Jastrow in their important paper (*On Small Differences in Sensation*; National Academy of Sciences, III. [1884]), which for the first time denied the supposed fact of the threshold.

[2] *On the Perception of Small Differences;* Univ. of Penn. Press, 1892. The present paper is largely based on this monograph.

entirely arbitrary to choose one difference and call it just noticeable. A difference in the stimuli can be found which will be obscured by the error of observation 1 time in 10, or 1 time in 1000, but no difference can be called just noticeable, meaning that it and larger differences will be correctly distinguished, while smaller differences will be indistinguishable. In actual experiments Prof. Fullerton and the writer found that the difference fixed on by the same observer under changed conditions as just noticeable was not at all proportional to the error of observation, and with different observers the difference which they considered just noticeable in no way measured their accuracy of discrimination. In the many researches in which the method of just noticeable difference has been used, the just noticeable difference fixed on by the observer has probably been determined partly by his general knowledge of his error of observation (the difference he would seldom mistake) and partly by association, he choosing an apparently equal difference.

The last application of the theory of probability which I wish to make concerns the relation of the error of observation to the magnitude of the stimulus. The algebraic sum of a number of variable errors tends to increase as the square root of the number. In measuring the base line of a survey the variable error of observation increases as the square root of the length of the line. It seems to me the same relation might be expected to hold in a general way when the length of a line is estimated by the eye or compared with another line. Or to take another example, if we estimate one second of time and repeat the trial four times, the algebraic sum of the four variable errors, or the combined error in estimating the four seconds, will tend to be twice as great as the error in estimating a single second. If we estimate or compare the four seconds continuously, the same elements would to a considerable extent be present, and we might expect an error twice as great as in estimating a single second—not four times as great as required by Weber's law.[1] The error in estimating each of the several seconds might and doubtless would be different, and in the case of intensive magnitudes equal objective increments would seldom or never be accompanied by equal changes in consciousness, nor be subject to equal and independent errors. The theory of probability only considers the simplest and most general case. We must use all the knowledge we have

[1] Constant errors increase in direct ratio to the magnitude, and would tend to follow Weber's law. But, curiously enough, constant errors have not been supposed by the psychologists to follow Weber's law. As a matter of fact "constant errors" are very inconstant and difficult to investigate.

as well as our theory, and the general formula must be adjusted to each special case.

In attempting to pull a dynamometer twice with the same force we do not compare the movements as we proceed, but the final result, and if the force were near the limit of our strength, the error might be less than for a smaller magnitude. We should expect a post-office clerk to judge very light weights better than a blacksmith, a blacksmith to judge heavy weights the better. We should expect to discriminate lights best within the range of ordinary daylight, and sounds best within the range of the human voice. Such results would be contrary to Weber's law, but are simply factors additional to the summation of errors required by the theory of probability. The relation between the error of observation and the magnitude of the stimulus will differ for each stimulus and for each observer, and will not remain constant even for the same stimulus and the same observer. But the usual increase of the error of observation with the magnitude of the stimulus is accounted for in a satisfactory manner by the summation of errors, and I should substitute for Weber's law the following: *The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case.*[1]

It may be asked if this view be correct, why do the results of researches confirm Weber's law? As a matter of fact Weber's law has not yet been confirmed exactly by any careful research, the error of observation usually becoming larger as the magnitude of the stimulus is taken larger, but almost always more slowly than in direct proportion to the magnitude. The attempt has been made by Fechner, Wundt, Helmholz and others to explain away the variations by additional hypotheses, but it is universally admitted that the validity of a law or hypothesis decreases as the number of subsidiary hypotheses increase.

I venture to think that it is an open question whether in the researches hitherto made the error of observation increases more nearly as the magnitude or as the square root of the magnitude. Researches in which the method of just noticeable difference has been used do not of necessity measure the

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[1] Prof. Fullerton pointed out at the meeting of the association that the conditions which made the first fractional or elemental error positive or negative might make the following error tend in the same direction. So far as such a tendency is present the error of observation would increase more rapidly than the square root of the stimulus, and more nearly in direct proportion to it (Weber's law).

error of observation at all. The variation in adjusting the just noticeable difference would roughly measure the error of observation, but this has been neglected. All the researches on lights with which I am acquainted[1] (excepting that by Prof. Fullerton and the writer) used the method of just noticeable (or more than noticeable) difference. Now it is natural enough (considering its elasticity) to make the just noticeable difference within the range of ordinary daylight proportional to the intensity of the light. We see the same objects more or less brightly illuminated, and should tend to regard the differences in shade and color as equal differences, whatever the intensity. It may also be remarked that the mechanism of the eye (accommodation of the pupil and sensitiveness of the retina) tends to obliterate objective differences in brightness. Further in all these researches on lights (excepting Merkel's) the lights were side by side, and the time of exposure was not limited. In such a case the error of observation becomes much obscured, and almost any result can be obtained.

I venture to maintain this conclusion even against the very careful research by König and Brodhun, which supports Weber's law for a considerable range of intensity. It is especially difficult to adjust a just noticeable difference when the areas of light are very small, and for colors not usually seen. König and Brodhun found the just noticeable difference for different colors of apparently the same intensity to be the same (ca. $\frac{1}{75}$ of the light). Previously with much the same methods Lamansky found the just noticeable difference for red $\frac{1}{70}$, for yellow and green $\frac{1}{286}$, for violet $\frac{1}{106}$, whereas Dobrowolsky found for red $\frac{1}{14}$, yellow $\frac{1}{46}$, green $\frac{1}{59}$, violet $\frac{1}{268}$-$\frac{1}{67}$. The three researches were carried out in Helmholz' laboratory, and we may well be at a loss to draw any conclusions from such discordant results.[2] Perhaps the two best researches with lights have been carried out by Aubert and by Müller-Lyer. Both of these writers think their results do not support Weber's law.

It is not necessary in this place further to review and compare results of researches on lights and other stimuli. If it be admitted that the just noticeable difference be not proportional to the error of observation, the amount of work to be

[1] Bouguer, Lambert, Arago, Masson, Fechner, Volkmann, Aubert, Helmholz, Plateau, Delbœuf, Kraepelin, Dobrowolsky, Lamensky, Breton, Ebbinghaus, Merkel, Lehmann, Neiglick, Schirmer, Müller-Lyer, König and Brodhun.

[2] More especially as Helmholz, in the revision of his *Physiologische Optik*, does not even mention Lamensky and Dobrowolsky. Nor does he refer to work not done in Berlin.

considered would be greatly reduced. Further, many researches by the method of average error and right and wrong cases have only a tolerable validity (*e. g.*, Fechner's and Merkel's) because the observer knew the relation of the stimuli before comparing them. In other cases (*e. g.*, with tastes, temperatures, touches and sounds), the stimuli have not been measured in a satisfactory manner. I believe the various researches are so disparate, having been made by so many observers (often young men working for a degree) and by such varying (and in many cases inadequate) methods, that the only general conclusion which can be drawn is that the error of observation tends to increase as the stimulus is made larger and usually more slowly than in direct proportion to the stimulus.

Before concluding I wish to notice the relation between the error of observation and the estimation of mental intensity. It has commonly been assumed that the variable error and the probable error (or *h* in Germany) are proportional to the just noticeable difference. The just noticeable difference has further been used to measure the intensity of sensation. The just noticeable difference is thus used ambiguously, on the one hand as a difference equally likely to be correctly perceived, on the other hand as a difference accompanied by an apparently equal increment in sensation. I entirely question the application of the error of observation to the measurement of the intensity of sensation. Supposing the intensity of sensation to be measurable, it may increase as the stimulus or (conceivably) as the logarithm of the stimulus, while the error of observation may be any other function of the stimulus[1] When it is evident that the error of observation may be increased or decreased in many ways without greatly altering the apparent intensity of sensation, I cannot understand how it has come to be used as a unit suitable for measuring the intensity of sensation. The error of observation is a physical quantity, a function of the intensity, area, duration, etc., of the stimulus, of the condition of the nervous system, and of the faculties, training, attention, etc., of the observer. That it should increase with the magnitude of the stimulus, and tend to increase as the square root of the magnitude, seems to me a natural consequence of the summation of errors. But I see no necessary connection between the supposed fact that the error of observation increases in direct proportion to the stimulus and the consequence which has been drawn from it that the intensity of sensation increases as the logarithm of the stimulus.

[1] This was noticed by G. E. Müller in 1879 (Zur Grundlegung der Psychophysic, p. 79-80).

The measurement of the intensity of sensation is not out of the question because the error of observation cannot be used as a unit. The attempt is made to accomplish this when for different intensities sensations are adjusted midway between two others, when they are made apparently half or double others, or, lastly, when they are made just greater or less than others in the sense that the difference in sensation is apparently equal. The question here is whether we do in fact judge differences in the intensity of sensations, or whether we merely judge differences in the stimuli determined by association with their known objective relations. I am inclined to think that the latter is the case. I find it comparatively easy to adjust one time, length of line or weight midway between two others, much more difficult to judge when one light or sound is midway between two others, and almost impossible to judge one temperature or pain midway between two others. The difficulty of making a decision increases as the objective relations are less familiar, and I believe that my adjustment is always determined by association with the known quantitive relations of the physical world. With lights and sounds, association might lead us to consider relative differences as equal differences, and the data would be obtained from which the logarithmic relation between stimulus and sensation has been deduced. With the force, extent and time of movement, Prof. Fullerton and the writer have shown that our estimates tend to follow the objective relations. But, in any case, if we merely judge the relations of objective magnitudes by association, we have no basis whatever for determining a relation between physical energy and mental intensity.

I conclude, consequently, that we cannot measure the intensity of sensation and its relation to the energy of the stimulus either by determining the error of observation or by estimating amounts of difference. The most natural assumption would seem to be that the intensity of sensation increases directly as the energy of the brain changes correlated with it. The relation between the energy of the brain changes and the physical stimulus is a physiological question. This conclusion does not mean, however, that psycho-physical research is valueless. On the contrary it is an important contribution to the science of psychology, whence its application will be extended to physical science, to art, to medicine, to pedagogy and in other directions.